SCIENCE

NEW SERIES
VOL. 83, No. 2147

FRIDAY, FEBRUARY 21, 1936

SUBSCRIPTION, $6.00
SINGLE COPIES, .15

Entered as second-class matter July 18, 1923, at the Post Office at Lancaster, Pa., under the Act of March 3, 1879.

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VOL. 83 FRIDAY, FEBRUARY 21, 1936 No. 2147

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS

New York City: Grand Central Terminal
Lancaster, Pa. Garrison, N. Y.
Annual Subscription, $6.00 Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

REPRODUCTION AND INHERITANCE IN ASCOMYCETES[1]

By Dr. B. O. DODGE

THE NEW YORK BOTANICAL GARDEN

IT is only within the last few years that students of the rusts and ascomycetes have obtained definite information about the processes actually involved in fertilization. Some of my associates have suggested that in view of this added knowledge it might be well to bring together in review on this program available information bearing on sex, maleness and femaleness as related particularly to heterothallism, incompatibilities and sterilities in the fungi. This did not seem to be advisable, however. Certainly by the time the naturalists hold their next regular quarter-century symposium on sex the mycologists will be in a better position to contribute positively to such a program. Although some of the topics presented for your consideration to-day may incidentally have a bearing on questions relating to sex in general, they are brought forward primarily to indicate some further profitable lines for thought as well as research in connection with the ascomycetes.

Muller believes that the essence of sexuality is Mendelian recombinations. While morphological differentiations are enhancing, they are secondary and dispensable. Allen has long studied sexuality and inheritance in the dioecious Bryophyta where differentiation of antheridial and archegonial haploid plants is clear cut and probably has a chromosomal basis. He would not be inclined to accept, as applying to his group, the theory of relative sexuality according to which all organisms, simple or complex, haploid as

[1] Address of the retiring vice-president and chairman of the Section for the Botanical Sciences, American Association for the Advancement of Science, St. Louis, December 31, 1935.

well as diploid, plant or animal, are potentially bisexual or hermaphroditic. Allen voices a view held very generally when he says that sex and sexuality imply advantageous morphological and functional differentiations. Link would also insist that without differentiation into male and female there can be no sex and therefore no sexual reproduction. We shall understand the situation in the ascomycetes better if we first take a few illustrations from other groups of the fungi.

In *Sporodinia* the fusing gametangia are exactly alike and there is nothing whatever to suggest male and female structures. According to your definitions the formation of zygospores would not be a case of sexual reproduction. In *Zygorhynchus heterogamus* there is a noticeable difference in size and position of the fusing cells. This is sexual reproduction, the large cell being female, the smaller one male. In *Absidia spinosa* a similar difference is found; but when they grow the *Absidia* against the *Zygorhynchus* or test them out in some other comparable way, large cell reacts with large cell, small with small, that is, female reacts with female, male with male. These interspecific matings never result in viable zygospores, but the reactions are very definite. Either difference in size here is not a mark of maleness and femaleness or this distinction has relatively little significance.

There is something here we are apt to overlook. The nuclei that come together in the zygospore of hermaphroditic species are exactly alike genetically, while the nuclei in the +/– gametangia of heterothallic species such as *Phycomyces nitens* may be genetically very different. It would be difficult or perhaps impossible to prove this in the zygomycetes, but it has been proved many times in the ascomycetes. Biochemical studies by Satina and Blakeslee provide no evidence at least against Blakeslee's original conception that – races are male and + races are female. These authors are not ready to insist that +/– differences have a chromosomal basis even in dioecious species. The reactions in cross-matings can not be due to the fact that the nuclei involved are unlike or that they are alike genetically. There is some other change here correlated with age or maturity. Zygospore formation in the first case would assist evolution, but it would not do so very much in the second. Certainly we should not use the symbols +/– to designate genetic nuclear qualities in one case and merely phenotypic cell differentiations in the other. It is on this account that I have not employed these signs to designate races of opposite sex-reaction in ascomycetes like *Neurospora* where the reaction has a chromosomal basis.

Arthur seems to have favored de Bary's views on the nature of spermogonia of the rusts, yet he chose to apply the term pycnium to the spermogonium and refers to its spermatia as pycniospores. This no doubt greatly influenced the younger generation to come to believe that spermatia wherever found are merely microspores. Craigie's discovery that in the rusts they actually function as fertilizing elements changed this view almost over night. The pycnium must after all be male and its pycniospores are proved to be spermatia. Morphologists never have questioned this.

Craigie, Ruth Allen and Hanna again disturb our notions, settled for a time, by proving that the paraphyses which often extend well out of the spermogonium are trichogynes down which spermatia nuclei travel to accomplish fertilization. The spermogonium is then hermaphroditic, male and female. If these paraphyses are female sex organs, what are those trichogynous hyphae that grow out through stomatal openings to receive the male nuclei? Andrus says they are female organs. Furthermore, they now claim that the uredospores can be made to function as fertilizing elements, and finally that the spermatia after all are merely microspores because they can germinate to form mycelia. All of which leads me to ask: Does the way an organ functions count only when its functioning furnishes the evidence we are looking for to support some particular morphological concept?

Bornet and Thuret's work on the nature of trichogynes and spermatia of the red algae stimulated much corresponding research on the primordia of the fruit bodies of the lichens and other ascomycetes. Stahl's and Baur's figures of septate trichogynes with attached spermatia are familiar to every one. One recalls Thaxter's many beautiful illustrations of trichogynes and spermatia of species of the Laboulbeniales. For a number of years only the followers of Brefeld and Dangeard doubted that trichogynes of these ascomycetes function to receive the male nuclei from spermatia and antheridia, especially as Harper had proved the case cytologically for *Pyronema*.

We must have female and male chlamydospores in heterothallic *Ascobolus carbonarius*. The female, borne on shorter thicker stalks, germinates to produce a complicated ascogonial coil ending in a septate trichogyne which contacts the male, raised on a long thin stalk and serving as an antheridium. The idea that "male" nuclei could ever reach the ascogenous cells when they would have to break through twenty or more cross-walls has never been considered very seriously, to say the least. Atkinson held that all such septate outgrowths from ascogonia represent simply a progressive sterilization. No one, he said, has ever proved that such outgrowths could possibly function. It will be shown later that cross-walls need offer little hindrance to passage of nuclei.

In *Ascobolus magnificus* the ascogonium tapers

gradually into a septate receptive end which seeks out the antheridium arising nearby. In this heterothallic species sex organs are not produced when races from single ascospores are grown alone. Yet as soon as two mycelia of opposite sex-reaction mix in a culture, anastomoses occur and antheridia and ascogonia appear, but always on different hyphal branches. It is in just such species that one should look for a close relation between the morphological differentiation of the reproductive structures and heterothallic nuclear sex-reactions.

As botanists continued to study ascomycetes culturally and no one demonstrated that spermatia actually functioned as male elements, it again became customary to refer to them as microspores or microconidia, especially as reports came in now and then of persons claiming to have germinated them. If not all spermatia germinate neither do the ascospores of all species germinate in cultures. Drayton, however, brings us back into line by demonstrating that the microconidia of *Sclerotinia Gladioli* do function in fertilization. Then we find the microspores of *Neurospora, Bombardia* and *Pleurage* doing the same thing. It is now generally accepted, and it would seem accepted again on the basis of *function,* that spermatia of all ascomycetes are male organs and ascogonia with their trichogynes are female.

The ascogonium of normal *Neurospora tetrasperma* does not differentiate a trichogyne. Both Colson and Schönefeldt have established this point cytologically. Each cell contains two sexually different kinds of nuclei from the beginning. It is only in its unisexual condition that trichogynes are formed, and then only after the incipient perithecium has reached some size. Furthermore, receptive hyphae often develop early from the base of the ascogonium to usurp the function of the morphological trichogynes, if there ever are such here. Trichogynes should develop whether they are needed or not if they are definite units.

There are still other features that should give occasion for thought. Spermatia of *Neurospora* are often borne on specialized compound spermatiophores, each of whose cells must be a spermogonium. The spermatia are pushed out through a very definite and characteristic cup-like aperture just as they are in *Pleurage anserina.* There is no question that they are highly differentiated organs morphologically and functionally, although in *Sclerotinia* spermatia may also bud out like a yeast directly from ascospores, conidia and hyphal cells. When spermatia are applied to unisexual incipient perithecia of *Neurospora, Bombardia* and *Pleurage* fertilization follows. Spermatia must be "male."

In *Neurospora* the most characteristic feature is the production of vast quantities of monilioid conidia. If you apply conidia of one reaction type to incipient perithecia of the opposite type they will also effect fertilization. They are clearly differentiated morphologically and functionally from the female ascogonial coils. In sexual reproduction they are enhancing, they are advantageous, and they are even more efficient than the spermatia themselves. Therefore, according to your definitions they must be male sex organs, so that we have two kinds of spermatia, microspermatia and macrospermatia. But one says these monilioid conidia germinate to propagate the species asexually. So do the microspermatia germinate, 100 per cent. if you give them sufficient time and the right environment. The mycelia they produce do not differ at all from those derived from the monilioid conidia or from the original parent ascospores.

Proof that spermatia are altogether dispensable is easily obtained from a study of plate cultures of the facultatively heterothallic species of *Neurospora* and *Gelasinospora.* None of our races of *G. tetrasperma* is known to produce spermatia, but all of them do produce incipient perithecia. Therefore, all races must be female, that is if incipient perithecia are marks of femaleness. It is certainly very difficult to find ascogonia in some cases, however. Hüttig would probably call such primordia male if no ascogonia could be discovered. There are no male races and no hermaphroditic races, however, according to our definition. Fertilization must be accomplished some other way than by spermatization. In their reactions in sexual reproduction all these facultatively heterothallic races fall into two groups. If two races which are opposite in their sex reactions are grown from opposite sides of a plate culture, the fruit bodies tend to be distributed in a rather definite pattern on one side of the plate. Under proper cultural conditions the mycelium from one side does not invade the other side to any great extent. It has been proved for these species that, following hyphal fusions at the line of contact, nuclei from one mycelium migrate through cell after cell to the opposite side, so that a mycelium that originally contained only one kind of nuclei will become heterokaryotic, bisexual.

In heterothallic species of *Spirogyra* and *Zygnema* it is generally agreed now that where the contents of a cell from one filament pass out through the conjugating tube over into a cell of the opposite filament in which the zygospore is formed, there is a sexual differentiation, a difference in activity. One cell is male, the other is female. According to this criterion our race Gel. 1 must be male because its nuclei move out from Gel. 1 over into Gel. 11. Buller has shown that in the hymenomycetes nuclear migration is possible because of the pores left in the cross-walls as the result of the ring type of septum formation known

to be characteristic of these fungi. Lehfeldt's idea that the cross-walls break down is probably incorrect.

Strain S_1 of *Neurospora tetrasperma* is male and S_9 is female if direction of moving nuclei is a basis for distinguishing the sexes. Certainly, most students will agree that the mycelia on which fruit bodies are formed must be female. But if you use a character for determining sex like that originally used by Blakeslee, then our tester race, S_1, which produces orange-colored conidia in much greater abundance than does tester race S_9, must be female instead of male. The point for thought here is that morphological differentiations of vegetative characters, differences in rates and directions of movements in nuclear migrations and the final location of fruit bodies are not true bases for distinguishing males and females, although according to your definitions they would be.

Neurospora Toroi is a species which comes from Puerto Rico. It manifests the same general type of perithecium distribution pattern, and its heterothallic races react strongly against corresponding races of *N. tetrasperma*. These two forms of *Neurospora* have about the same morphological characteristics so that it would be difficult to tell them apart. The best evidence that they are two distinct species is that when they are mated the F_1 asci are seldom formed. Similar sterilities are always found in other interspecific hybrids of this genus.

To speak of the reactions of these races as sexual is not as inconsistent or as erroneous or as absurd as it is to use the terms male and female the way we do in these low plants, merely on the basis of very weak analogies. As others have said, we can not be consistent and we do not need to be where sex, as we call it, has probably arisen many different ways. To insist that these reactions are merely incompatibility relations is misleading because here they are nuclear. To say that this is what incompatibility should mean is begging the question because no plant breeder or horticulturist uses the term that way. We may illustrate the confusion that would arise from the use of this term by comparing certain interspecific reactions. We have three obligately heterothallic species of *Neurospora*. If we find that race A of *N. intermedia* from China produces fertile fruit bodies when grown with race B of *N. sitophila* from France, and this race B also produces fertile perithecia when mated with race C of *N. crassa* from Louisiana, and if we express these relations in terms of incompatibilities, we can not know in advance of actually proving the case by culture tests what will be the reaction between race A and race C. But if we say A and B are of opposite sex because they produce perithecia when mated, and race B is opposite to race C for the same reason, every one knows that races A and C are alike sexually so that no fertile perithecia need be expected when they are grown together.

It would be difficult to prove, but we should like to know whether in the dioecious red algae there is ever a case where the male plants of the species can be divided into two groups so that all the individuals in one group are cross-sterile or incompatible with the individuals of a corresponding group of female plants, while the plants of the second male group are compatible with these same females. In dioecious Bryophyta no such bipolar intersterilities have ever been reported. Furthermore, no case is known in *Sphaerocarpos*, Professor Allen informs me, where otherwise normal plants of a male clone are cross-sterile with normal plants of a female clone.

It is curious that certain groups of these fungi, otherwise so irregular and primitive, should have worked out so early the most perfect of all devices to prevent self-fertilization, that is, the differentiation of their nuclei genetically into two different kinds with respect to their reactions in sexual reproduction. One wonders how it happens to be that this very striking bipolar interaction should exist particularly in rusts and ascomycetes where differentiation of the sexes morphologically has not become stabilized and where, if I may be allowed to express it that way, all sorts of devices are being tried out to effect fertilization.

Morphologically speaking, I would be the last to deny that heterothallic races of *Neurospora*, for example, are potentially bisexual or hermaphroditic, with enhancing yet dispensable trichogynes and spermatia. Such races are, nevertheless, fundamentally and indispensably unisexual for purposes best adapted to evolution.

There are several other terms such as sporophyte, diploid, diploidization, hybrid and hybridization that are frequently used very loosely and inconsistently when applied to the fungi. If any one is able to work out a terminology that will more exactly express the situation in these fungi instead of adding to the confusion with glittering generalities we shall all be grateful. In the meantime, if we must continue to be inconsistent, let us be inconsistent where it will serve a useful purpose.

Hüttig very cleverly avoids the dilemma by boldly asserting, without much evidence, that all his self-fertile races of *Glomerella Lycopersici* are hermaphroditic and all self-sterile races are male, and goes on with his interesting breeding work just as though he were dealing with strictly heterothallic species. He has three linked factors, the first two of which, α and γ, are "realizators" and control sex-reactions. The third factor β is physiological and influences copulation. This author seems to be unaware of Edgerton's prior work on *Glomerella*. He thinks he has in this

species evidence in support of the theory of relative sexuality. Relative, however, refers to relative degrees of fertility and not to relative morphological differentiations of the organs themselves. May we not nevertheless accept the theory of relative sexuality as well as the idea of incompatibility for what they are worth in these fungi and not let terminology interfere with productive research.

The first fertile hybrid ascomycetes developed artificially were obtained about ten years ago by crossing a four-spored species of *Neurospora* with an 8-spored species. They are the only interspecific hybrid ascomycetes that have been produced, yet either because the F_1 asci did not show six spores, the number intermediate between 4 and 8, or because races obtained by germinating the f_1 spores were not pure for either 4-sporedness or 8-sporedness, that work has been entirely ignored. Not so when a comparatively simple and easy cross between an albinistic non-conidial mutant race and the normal orange-colored race of the 8-spored species was reported. Of the eight f_1 spores from an ascus four gave rise to albinistic non-conidial mycelia and four to orange-colored, a perfect Mendelian segregation. This is what we wanted to see, if the same rules of inheritance govern the fungi and the higher plants and animals. The ignored interspecific hybrids are more interesting and important, however.

To illustrate how beautifully some of these ascomycetes are adapted to demonstrate Mendelian segregation one needs only to culture *Bombardia lunata,* according to Zickler. Here the characters *viridis* and *rubiginosa* are expressed not only in connection with the mycelia but also by the ascospores while they are yet within the ascus, four of each kind, variously arranged accordingly as segregation for these factors occurs in the first or in the second division. Culturing from the ascospores it was proved that these factors are linked with the sex-reaction factors.

Sectoring which shows variation in the type of mycelial growth and sporulation has usually been interpreted to represent a mutation if the variant remains constant through vegetative propagation. If such mutants could be carried through sexual reproduction, however, we must expect an occasional reversion to normal. This could be proved by analyzing the eight spores from individual asci. In some species there would be a characteristic arrangement of the spores carrying the factors concerned.

Zickler has recently proved that mutants arising as sectors in plate cultures of *Bombardia* are genetic by analyzing progeny obtained by crossing the variants with the normal wild type. In some cases there was the tendency to revert to normal, which was easily seen because in the races *viridis, rubiginosa* and *lactea* the ascospores themselves reflect by their color their genetic constitution. He observed a 2:6 arrangement of spores, an arrangement which Lindegren and Wülker have also interpreted to represent reversions in cases of mutant strains of *Neurospora* which they have studied.

Not all sectoring need denote mutation. Albino spermatial strains of *Pleurage anserina,* which had first arisen as sectors from very dark-colored non-spermatial races, have themselves sectored to show differences in the abundance of spermatia. Breeding tests indicate that the phenomenon here may be in part merely phenotypic, at least the subsectoring of the albino was not due to a mutation.

If one crosses an albino non-conidial race of *Neurospora sitophila* with a conidial race of *N. crassa* and then back-crosses two or three times he will obtain asci in which all eight spores mature. One usually finds a clear-cut segregation, four non-conidial and four conidial. Sometimes, however, two of the albinos develop a few conidia (something that does not occur if you make the cross between conidial and non-conidial races of the same species). In other cases four of the albinos will show a few conidia. Geneticists would no doubt interpret the phenomenon as a partial reversion, or a reversion of one of the factors concerned in conidium production.

We have developed as the result of x-ray treatment races the asci of which frequently abort without spore formation. In one case the abortion is apparently due to an incompletely dominant lethal gene. The nuclear fusions occur as usual, but after three or four successive nuclear divisions the ascus contents degenerate. The ascus wall then takes over the differentiations characteristic of the ascospores themselves, and persist as striated olivaceous-brown sacs. All asci in every perithecium are heterozygous for this lethal. Some asci succeed in cutting out spores, but spores carrying only lethal nuclei die after germination. Asci homozygous for the lethal can not be obtained. It is interesting to see that when a lethal nucleus is associated with a normal one in the same cytoplasm, it divides continuously and is carried along until it fuses in the ascus with a normal nucleus.

The type of ascus abortion just described is genetic, but a similar though non-heritable type can be induced both by chemical treatment and as the result of certain abnormal cultural conditions.

We have another type of ascus abortion which occurs when asci are homozygous for a recessive lethal gene. Here no ascospores are ever cut out and the asci finally disintegrate. Asci heterozygous for the lethal form spores normally, but each spore will be provided with one lethal and one normal nucleus. Mention was previously made of relative amounts and coloration of

masses of conidia produced by unisexual races of *Neurospora tetrasperma*. The presence of this recessive lethal l, in race S_1 for example, so inhibits the factor 0 that the S_1 race looks like the S_9 race, although the sex-reactions are not at all affected by the lethal for ascus abortion.

In the 8-spored species of *Neurospora* the nuclear spindles during reduction division in the ascus are so oriented that segregation of any pair of factors in the first division is manifested by the arrangement of the spores in the ascus, 4 and 4, for these characters. If the segregation occurs in the second division this is manifested by the alternation of the spores 2 and 2, or 2, 4, 2. Should two or more pairs of factors be involved and one pair segregate in the first division and another pair in the second division, then we shall have four kinds of spores formed in an ascus.

Bridges has suggested that where in any case four kinds of spores or gametes are formed from a mother-cell following reduction division this could be due to crossing-over. Lindegren analyzed over 100 asci obtained in crosses involving two pairs of linked factors, +/− for sex-reaction and P/p for mycelial characters. He obtained about 22½ per cent. recombinations. He also found that about 13 per cent. of the asci showed segregation for the +/−, and 33 per cent. for the P/p factors in the second division. If a second division segregation indicates a simple cross-over, Lindegren argues, second division percentages could be used to compute chromosome map distances. Map distance units obtained through actual crossing-over percentages (indicated by second division percentages according to Lindegren) are always twice as large as standard units computed on the basis of recombination percentages. Since the distances between +/− and P/p computed these two ways independently agree (22.5 : 23) Lindegren believes he has proved his assumption which was that the spindle fiber attachment points (S. F. P.) always reduce in the first division just as they do in *Drosophila*. Post-reduction, meaning separation of non-sister chromatids in the second division, does not occur in *Neurospora*. Such logic is correct only if second division segregations can not be explained in any other way.

Zickler, analyzing over 8,000 asci of *Bombardia lunata*, finds that crossing-over does not account fully for the number of second division segregations of his linked factors. One must assume, he says, that any two of the four chromatids may move back together during the first division, a pair to each pole, so that two thirds of the time there could be reduction of the S. F. A. in the second division.

Wülker takes a middle ground view to explain his second division segregations. His results (180 asci analyzed) prove, he thinks, that there must be a side by side working of both crossing-over and post-reduction. It is clear that further work along lines first suggested by Lindegren is necessary to settle this question, which it seems is a very important one from a genetic standpoint. It will be difficult to settle it cytologically in case of *Neurospora*.

In *Peziza subumbrina*, which Matsuura and Gondō studied, there is regularly found at reduction a pair of heterochromosomes, one a long rod, the other a small spherical body, suggesting that the species may be heterothallic and that this pair of chromosomes may be the mechanism governing the sex-reactions. It is interesting that the authors sometimes find this pair of chromosomes splitting equationally in the first division, non-sister chromatids, one long and the other short, going back to each pole. They believe that the other chromosome pairs may also split equationally in the first and reduce in the second division, but this can not be determined cytologically because of their small size.

Whether or not Lindegren's views that the S. F. A. always reduce in the first division will apply to these ascomycetes, he has certainly given us an excellent set of models for recording and interpreting our culture results genetically.

Glomerella Lycopersici, it would seem, must be a type intermediate between the strictly heterothallic and the homothallic species. Hüttig isolated several kinds of haplonts which vary in their degree of fertility ranging from strongly self-fertile hermaphroditic clones to those that are wholly "male" and completely self-sterile. The results upon which he bases his conclusions were obtained by analyzing ascospores collected at random from cultures in which his self-fertile and his so-called self-sterile or "male" races were mixed. Any one who knows these ever-mutating Glomerellas realizes that such results can have little genetic significance. The mechanism by which fertilization occurs must be unique, because, as is well known, races heretofore self-sterile may become self-fertile when grown against another race, merely because of the altered nutritional conditions. If, however, Hüttig will analyze the eight spores from individual asci and obtain comparable results he will have proved *Glomerella* to be one of the most interesting ascomycetes yet studied genetically.

Mycologists who undertake the study of ascomycetes or other fungi genetically will always be handicapped by their lack of that training which is necessary for adequate analysis of any extensive breeding work. Our proper function is to remain mycologists. We must discover for the geneticist species which are well adapted not only morphologically but culturally for their type of work. We should develop the culture technique and work out the morphology and the details relating to propagation and reproduction, and

prove by our preliminary hybridization work that there is need for a further genetic study of the species. On the other hand, geneticists without mycological training undertaking to study the ascomycetes should realize that the ways of these fungi are devious and beset with pitfalls. Nevertheless, it is encouraging to know that fundamentally in their reproduction and inheritance the fungi follow exactly the same laws that govern these activities in the higher plants and animals.

A GREAT PUPIL AND A GREAT DISCOVERY—BOTH SUPPORTED BY A GREAT TEACHER

By Dr. HARRY N. HOLMES

OBERLIN COLLEGE

ON this February 23d, it will be exactly half a century since an Oberlin College student, young Charles Martin Hall, gave aluminum to the world as an industrial metal.

This brilliant discovery of the present industrial process of making aluminum was no accident but the result of long-continued effort and intelligent planning—in cooperation with one of the best-trained teachers of chemistry in America, Frank Fanning Jewett.

Hall at 22 succeeded where many of the greatest scientists of his century failed. It is true that Oersted, the Dane, famed for his fundamental research in electricity, was the first to isolate aluminum (1825) and that Wöhler, the German, dominant authority in chemistry, improved slightly upon Oersted's method (1827), wrongly receiving world credit for the discovery; but these pioneers saw only a little black powder instead of shining, massive aluminum. The scientific world was thrilled, but industry was not greatly benefited.

The following equation represents the method by which this metal was isolated:

$$AlCl_3 + 3K \rightarrow 3KCl + Al.$$

The eminent French chemist, Deville, complaining that aluminum was still slightly more expensive than silver, lowered the cost in 1854 by the simple substitution of the cheaper metal sodium for potassium in attack on a mixture of aluminum chloride and sodium chloride.

$$AlCl_3 + 3Na \rightarrow 3NaCl + Al.$$

Within two years, by 1856, the price of the metal dropped from $90.00 per pound to $27.00.

Sir Humphry Davy made earlier attempts than Wöhler's to reduce the oxide and failed, as did Silliman. Berzelius, the eminent Swedish chemist, almost succeeded in anticipating the success of Wöhler when he heated cryolite, the double fluoride of aluminum and sodium, with potassium. Unfortunately, he used an excess of potassium and got an alloy of aluminum with potassium. Had he used an excess of cryolite, Berzelius would now be given credit for presenting aluminum to science. Deville actually gave them all a start on the right track in another method that failed. He electrolyzed melted cryolite, a double fluoride of aluminum and sodium, but the results were unsatisfactory. Bunsen is said to have done the same thing at the same time. Half a century later these unsuccessful experiments were dragged into court in an effort to deny Charles M. Hall the fruits of his own great discovery.

Deville worked hard to cheapen the cost of the necessary sodium for his reduction process and actually founded a small industry. Heating sodium carbonate (and some calcium carbonate) with carbon he secured metallic sodium in commercial quantities.

Then came Castner, who, with admirable clearness, saw that cheaper sodium meant cheaper aluminum. With equally admirable directness he proceeded to devise a cheaper process of making sodium and at once cut the cost of aluminum to the encouraging figure of $6.00 (later, $4.00) a pound. Then, just at the moment of reasonable success, poor Castner was flattened by the news of young Hall's great discovery!

Now, in 1880, enters upon the scene a quiet, studious lad, Charles Martin Hall, son of a minister in the village of Oberlin, Ohio. Dreaming of his "schemes" to make great discoveries for humanity, actually making a real invention at 17 and finding it already patented, reading in a dog-eared old chemistry of the properties of aluminum, he was ready to enter Oberlin College in 1880.

It was most fortunate for Hall and for Oberlin College that in 1880 Frank Fanning Jewett accepted the chair of chemistry and mineralogy, bringing to the work a training equal to the best of that time. A Yale graduate, he had gone to Germany, where, in Göttingen University, he was one of the small group of American students who at that time specialized in chemical work under highly trained German teachers. Among his close friends at Göttingen were Provost Edgar F. Smith, of the University of Pennsylvania,

Professor Harmon Morse, of Johns Hopkins University, and Professor Mears, Sr., of Williams College. After his return from Germany Professor Jewett served as research assistant to Wolcott Gibbs of Harvard and, later, for a period of four years as professor of chemistry at the Imperial University of Japan.

On the occasion of the fiftieth reunion of his Yale class (1920) Professor Jewett, like all his classmates, was asked to give an accounting for his 50 years of activity since leaving Yale. His remarks are worth presenting here as valuable historical material.

My great discovery has been the discovery of a man. When I went to Oberlin in 1880, on my return from four years' teaching in Japan, there was a little boy about fifteen years old who used to come to the chemical laboratory frequently to buy a few cents worth of glass tubing or test tubes or something of that sort and go off with them. He would come again after a while to get some more things to work with.

Not knowing anything about the boy I made up my mind that he would make a mark for himself some day because he didn't spend all his time playing but was already investigating. That boy was Charles M. Hall, the man who, at the age of twenty-one, discovered the method of reducing aluminum from its ores and making it the splendid metal that we now see used all over the world. Hall was an all-round student, but he did have a special liking for science.

After he had entered college and was part way through the regular course, I took him into my private laboratory and gave him a place by my side—discussing his problems with him from day to day.

Possibly a remark of mine in the laboratory one day led him to turn his especial attention to aluminum. Speaking to my students, I said that if any one should invent a process by which aluminum could be made on a commercial scale, not only would he be a benefactor to the world but would also be able to lay up for himself a great fortune. Turning to a classmate, Charles Hall said, "I'm going for that metal." And he went for it.

He tried various methods in vain, and finally turned his mind to the idea that perhaps electricity would help get the metal out of its ores. So he focused his attention on that process. I loaned him what apparatus I had to spare, what batteries we could develop. And I think that most of you who have seen an electric battery would have laughed at the one we got up—made as it was out of all sorts of cups, tumblers and so on, with pieces of carbon in them. But we finally got the current that was needed.

Soon after this he was graduated and took the apparatus to his own home; apparatus which he himself made and which I had loaned him. He arranged a little laboratory in the shed, continued his investigation and reported to me frequently.

About six months later he came over to my office one morning, and holding out his hollowed hand said: "Professor, I've got it!" There in the palm of his hand lay a dozen little globules of aluminum, the first ever made by the electrolytic process in this country. This was the 23rd of February, 1886. After that he developed his invention to its final great success.

Some further reminiscences of Professor Jewett's, written in 1914, are of great importance in throwing light on the relation of teacher to pupil and to discovery. It will be seen that Jewett inspired, advised, helped and encouraged his pupil Hall, taught him all the chemistry the boy knew and gave him the priceless privilege of a place in the master's private laboratory. Jewett did not make the discovery but he was necessary to it.

Even before he entered college, the subject of extraction of aluminum from its ores had occupied his mind. During his college course, even while engaged in regular chemical studies, he did some investigating on the subject.

At one time, he suggested that he and I should undertake to find a better material than carbon for the fiber in the incandescent lamp. He concluded that tungsten would answer. It was agreed that I should furnish the materials and that he should do the work in my private laboratory. Here he had his own desk, which he continued to use during his senior year. He worked with tungsten compounds for a season and finally found one which we thought might answer the purpose. When a fiber made of this tungsten was subjected to as strong a current as the laboratory afforded it glowed brightly for an instant or two, then snapped asunder. It was planned to take up the subject later, but circumstances would not permit. (Had they stuck to tungsten the tungsten lamp might have arrived twenty years earlier.)

Mr. Hall first tried to extract aluminum from clay by fusing it in a crucible with carbon and chlorate of potash, but nothing came of it. Later he tried to get aluminum from its oxide by fusing with different substances. Toward the end of his college course he finally abandoned altogether the idea of securing aluminum by reduction, and turned his thoughts extensively to electricity as the form of energy to be employed in separating aluminum from its compounds.

In talking the matter over he asked me at one time regarding the compound which could be most readily decomposed by the electric current, knowing that in gold and silver plating the cyanides of those metals were used. I suggested such a compound for aluminum. We soon concluded, however, that this would be impracticable on a large scale, and we next considered the fluoride. Lead dishes were now supplied in which to make these substances, and he worked assiduously for many days in preparing them. When a satisfactory compound was finally made the next step was to prepare an electric battery that would furnish a current strong enough to decompose it. All the battery cells that the laboratory could supply were pressed into service. This battery was then enlarged by additional cells made out of beakers, tumblers, jars, and everything else that ingenuity could devise, all

to no purpose, however. The electric current was not strong enough to do the work.

Soon after this Mr. Hall was graduated from college, and in 1886 fitted up a laboratory in a shed at the rear of his father's house on East College Street. Thither he carried the apparatus loaned him from the laboratory, and continued his work, occasionally coming to speak of his progress, and to talk about the difficulties which arose at almost every step of his investigation. These difficulties, however, never quelled his enthusiasm nor disheartened him.

The essential features of Hall's invention may be presented briefly in his own words, spoken on the occasion of the Perkin Medal Award.[1]

I had studied something of thermo-chemistry, and gradually the idea formed itself in my mind that if I could get a solution of alumina in something which contained no water, and in a solvent which was chemically more stable than the alumina, this would probably give a bath from which aluminum could be obtained by electrolysis.

In February, 1886, I began to experiment on this plan. The first thing in which I tried to dissolve alumina for electrolysis was fluorspar, but I found that its fusing point was too high. I next made some magnesium fluoride, but found this also to have a rather high fusing point. I then took some cryolite, and found that it melted easily and in the molten condition dissolved alumina in large proportions. I rigged up a little electric battery—mostly borrowed from my professor of chemistry, Professor Jewett, of Oberlin College, where I had graduated the previous summer. I melted some cryolite in a clay crucible and dissolved alumina in it and passed an electric current through the molten mass for about two hours. When I poured out the melted mass I found no aluminum. Then it occurred to me that the operation might be interfered with by impurities, principally silica, dissolved from the clay crucible. I next made a carbon crucible, enclosed it in a clay crucible, and repeated the experiment with better success. After passing the current for about two hours I poured out the material and found a number of small globules of aluminum. I was then quite sure that I had discovered the process that I was after.

In the summer of 1888, after most discouraging efforts to secure business support of his work, Hall and his patent were taken into association with Captain Roy Hunt and other "capitalists" of extremely modest means as the "Pittsburgh Reduction Company." This name, a few years later, was changed to "The Aluminum Company of America."

Hall's commercial success brought infringement suits by the Cowles Electric Smelting and Aluminum Company of Lockport, N. Y., in 1893. It is to be noted that Hall never entered their employ until *after* his discovery and also that he gave the Cowles Company an option on his process, which they finally rejected. The Cowles people reduced aluminum oxide with carbon in the electric furnace but were forced to use copper to capture the aluminum. This was purely a high temperature reaction, not electrolytic, and produced only a copper-aluminum alloy. The temperature of reduction of aluminum oxide is close to the boiling point of the metal.

Judge Taft, later President Taft, decided in favor of Hall, in the U. S. Circuit Court of Northern Ohio. It was necessary in the trial that the date of Hall's discovery be set before the summer of 1886 and this Professor Jewett was able to do because he remembered clearly that he stood in his private laboratory in the north wing of old Cabinet Hall when the young discoverer rushed in with a few shining buttons in his hand and said "I've got it!" This north wing of Cabinet Hall was torn down in the summer of 1886 to make room for the south wing of the new Peters Hall.

It is time that the world look past the spectacular figure of every genius to find, in his shadowy background, unknown to the public, some inspiring teacher. Unfortunately, the genius may fear that tributes to the old master may detract from his own glory. On the contrary, nothing does a man more credit than gracious acknowledgment of his scholarly debts.

[1] See "The Perkin Medal Award," *J. Ind. Eng. Chem.*, 3, 143–51 (March, 1911).

SCIENTIFIC EVENTS

PROGRAM OF THE AMERICAN WILD LIFE CONFERENCE

IRA N. GABRIELSON, newly appointed chief of the U. S. Biological Survey, outlined a proposal for a national program at the recent meeting of the North American Wild Life Conference. It is expected that the program to be worked out by the General Wild Life Federation, the organization formed by the conference, will be based on his suggestions. A summary of his requirements follows:

1. Land for the restoration and use of wild life. The Federal Government has a national responsibility to complete a migratory waterfowl program and, where necessary, to develop primary areas for the preservation of other wild life.

2. Closer cooperation between federal and state agencies. (a) By extending cooperative research and demonstration units now operating in nine states. (b) Federal

cooperation with states even to financial aid, in developing state wild life management areas that will supplement the primary federal areas.

3. Recognition of wild life values. Land management agencies, both public and private, should be brought to recognize the value of wild life and induced to provide for its needs so far as compatible with other uses.

4. Pollution of waters must be stopped or reduced to a non-destructive point.

5. Research on wild life problems should be extended to meet the new problems produced continually by modern developments. The results of this research should be freely available to all land administrators and wild life management agencies.

6. Closer coordination of the activities of federal land administrative agencies.

7. Basic protective legislation and regulations drawn to meet the needs of wild life, not merely the wishes of groups of special interests.

At the last session of the conference it was announced that a migratory-bird treaty between the United States and Mexico had been signed. The announcement was made by Juan Zinser, director of the Mexican National Game Commission, who said the treaty had been signed by Josephus Daniels, American Ambassador to Mexico, and General Eduardo Hay, Mexican Minister of Foreign Affairs. The provisions will come into force when the treaty is ratified by both countries and will remain in force for fifteen years, after which time it may be denounced by either country on twelve months' notice.

DU PONT FELLOWSHIPS FOR RESEARCH CHEMISTS

NOTING an increasing demand for research chemists, with a very definite shortage, and wishing to encourage more promising students in research work in the field of chemistry, E. I. du Pont de Nemours and Company has again appropriated sufficient funds to permit establishing fellowships in twelve leading universities and colleges for the academic year 1936–1937. The purpose of the plan is to give assistance to students wishing to pursue graduate work in research. The plan of these fellowships is very similar to the one the du Pont Company placed in operation many years ago but discontinued in 1932. At that time there was an excess of young research chemists with no offers of employment. As business conditions have improved, industrial research has been resumed with renewed interest, with the result that there is now keen competition for men of outstanding ability while the supply is becoming inadequate for the demand. These fellowships in the past have enabled young men to continue graduate work in chemistry who otherwise would have found it impossible to go on. Since the company first began these awards there have been granted 326 fellowships and 34 scholarships in 31 institutions, and, in addition, there was a national fellowship awarded at the Johns Hopkins University for a period of four years.

The appropriation for the resumption of the fellowships is $18,000, half of which is to cover the cost of continuing for the academic year 1936–1937 four post-doctorate fellowships in organic chemistry at $2,000 each, plus an additional $1,000 to cover the cost of extraordinary equipment that may be required in connection with the work of this group. The remaining $9,000 is to cover the cost of reestablishing twelve post-graduate fellowships at $750 each.

The objective of the post-doctorate fellowship is to provide trained assistants for a few of the younger professors of organic research to enable them to attack the more difficult type of problems, and to develop men who will be better qualified in research to continue their efforts in the academic field. The post-graduate fellowships, on the other hand, assist promising young men to obtain an education along the lines required by the chemical industry.

The awards have no restrictions other than that the work done under them shall be in the field of chemistry or chemical engineering, but the appointment of the fellowship must be approved by a member of the fellowship committee of the du Pont Company, after reviewing the qualifications of the appointee and the recommendation of the department of chemistry.

The twelve universities selected are as follows: For chemistry—University of Chicago, Cornell, Harvard, the Johns Hopkins, Ohio State, Princeton, Yale, Illinois, Minnesota, Wisconsin. For chemistry or chemical engineering—University of Michigan and Massachusetts Institute of Technology.

ALUMNI RESEARCH FOUNDATION OF THE UNIVERSITY OF WISCONSIN

A GRANT of $138,000 from the Wisconsin Alumni Research Foundation to aid research in the natural sciences at the University of Wisconsin was recently accepted by the State Board of Regents. The funds will support both old and new research projects. These projects, about eighty in number, are selected and approved by the University Research Committee. The foundation which provides the funds has no voice in the selection or in the policies to be followed in carrying out the research work.

"This grant," President Glenn Frank is reported to have said, "is another visible evidence of the very great contribution the Alumni Research Foundation is making to the future of the University of Wisconsin. By its accumulation of a permanent endowment for research, the foundation is providing an element of stability to the scientific future of the university that would otherwise be impossible. And in a dozen other

ways its grants are enriching the staff and work of the university. Its stimulation is being felt all the way from promising young scholars to our most distinguished scientists of maturity."

Of the total grant, $84,000 is allotted to special grants-in-aid to stimulate university research. These are used to purchase equipment and supplies and to help to support more than a hundred young men and women graduate research workers. Included in the grant is a new fund of $25,000 to permit faculty members to carry on during the summer months certain lines of research which are already underway.

There is also included a $15,000 fund for the continuation of the special fellowships and scholarships inaugurated a year ago, and known as the Wisconsin Alumni Foundation Fellowships for gifted young scholars and students of science. Funds for their support this year have been increased $5,000 over the $10,000 given for them last year. At the present time twenty-three young men are carrying on research work under these fellowships. They are selected for their attainments from all parts of the country.

An additional fund of $5,000 is included in this year's grant for the establishment of two or more post-doctorate fellowships with which it will be possible to bring to the state university gifted men who have already proved their ability to carry on independent research work in the natural sciences.

The grant also includes $8,000 which will provide for the continuation of the work now being done by Professor Aldo Leopold on game management and the waste land problems and $1,000 for the continuation of the lectureship fund, which each year brings to the university a leading investigator to lecture on some phase of the natural sciences.

THE TENTH ANNUAL PRIESTLEY LECTURES

DR. WARREN K. LEWIS, professor of chemical engineering at the Massachusetts Institute of Technology, will be the tenth annual Priestley lecturer at the Pennsylvania State College. The lectures will deal with the interrelationships between physical chemistry and chemical engineering, and will be given in the chemistry amphitheater of the college, at 7:00 P.M., on March 23, 24, 25, 26 and 27.

Professor Lewis, Perkin medallist for 1936 and one of the founders of chemical engineering, has chosen for his topic, "The Borderline Between the Physical Chemistry of Fluids and the Behavior of Suspensions." The five lectures will deal with the structure of liquids, the viscosity of fluids, suspensions and emulsions, and gelation.

The Priestley lecture series was inaugurated by the faculty of the department of chemistry in 1926 as a memorial to Joseph Priestley, the discoverer of oxygen, whose American laboratory was situated only a few miles from State College. In 1931 the Penn State Chapter of Phi Lambda Upsilon (Honorary Chemical Society) undertook the financial support of the lecture series. These lectures, therefore, now constitute a joint memorial to Joseph Priestley on the part of both the faculty of the department of chemistry and of the Penn State Chapter of Phi Lambda Upsilon.

The Priestley lectures deal each year with the borderline between physical chemistry and some other branch of science. Previous Priestley lecturers and their borderline topics are:

1927—Dr. V. Cofman, E. I. du Pont de Nemours and Company—Biocolloids.
1928—Dr. S. L. Hoyt, General Electric Company—Metallurgy.
1929—Dr. H. B. Williams, Columbia University—Medicine.
1930—Dr. L. Navias, General Electric Company—Ceramics.
1931—Dr. J. W. Williams, University of Wisconsin—Electrical Engineering.
1932—Dr. V. K. LaMer, Columbia University—Biochemistry.
1933—Dr. E. R. Jette, Columbia University—Metallurgy.
1934—Dr. R. A. Gortner, University of Minnesota—Life Processes.
1935—Dr. M. A. Hunter, Rensselaer Polytechnic Institute—Electro-metallurgy.

RECENT DEATHS

THE death is announced of Dr. William G. Krauss, professor emeritus of tropical medicine at the Medical College of the University of Tennessee, formerly director of the city laboratory of Memphis. He was one of the first of the southern medical profession to recognize the clinical value of x-rays. Dr. Krauss died as a result of his early work with x-rays.

JAMES H. SCARR, head of the United States Weather Bureau in New York City for the last ten years, died on February 14 at the age of sixty-nine years. Mr. Scarr was a fellow of the American Meteorological Society, scientific member of the Institute of Aeronautical Sciences and member of the American Association for the Advancement of Science.

DR. JAMES HARTLEY ASHWORTH, professor of natural history at the University of Edinburgh, died suddenly in Edinburgh on February 4 at the age of sixty-one years. Dr. Ashworth was visiting professor at the University of California in 1930.

PROFESSOR ANDREW FRANK DIXON, who had been

professor of anatomy and chirurgy at the University of Dublin for the past thirty-three years, died on January 15, at the age of sixty-seven years.

THE Peking correspondent of the London *Times* reports that the Chinese geologist, Dr. V. K. Ting, has died at Changsha at the age of forty-eight years.

SCIENTIFIC NOTES AND NEWS

THE doctorate of laws was conferred on February 14 by Dartmouth College on Dr. James Rowland Angell, president of Yale University, on the occasion of the celebration of the fortieth anniversary of "Dartmouth Night." Dr. Angell and President Ernest M. Hopkins, of the college, were the principal speakers. Until 1920 Dr. Angell was professor of psychology and dean of the faculties of arts, literature and science at the University of Chicago and was later chairman of the National Research Council and president of the Carnegie Corporation.

DR. FREDERIC EUGENE IVES, known throughout the world for his invention of photoengraving and color photography, celebrated his eightieth birthday at his home in Philadelphia on February 17.

AT the annual dinner of the American Institute of Mining and Metallurgical Engineers on February 19, the William Lawrence Saunders Medal was presented to Clinton Crane, president of the St. Joseph Lead Company of St. Joseph, Mo.; the Robert W. Hunt Prize was given to C. C. Henning, of the Jones and Laughlin Steel Company of Pittsburgh, and the J. E. Johnson, Jr., Award was presented to Francis Hearne Crockard, of the Republic Steel Corporation. Dr. Robert Peele, professor emeritus of the Columbia University School of Mines, was named as an honorary member of the institute.

A MEDAL for "conspicuous alumni service" was awarded at the annual Lincoln Day celebration of the alumni of Columbia University to Dr. William H. Woglom, member of the medical class of 1901 and professor of cancer research at the Crocker Institute.

AT the recent annual meeting of the Geological Society of America, Dr. Victor M. Goldschmidt, professor of mineralogy and geochemistry at Göttingen; Dr. Paul Fourmarier, professor of commercial geography at Liége, and Dr. Emile Argand, professor of geology and paleontology at Neuchâtel, were elected foreign correspondents.

THE council of the Geological Society, London, has made the following awards: The Wollaston Medal to Professor Gustaaf Adolf Frederik Molengraaff, of Delft, in recognition of his researches concerning the mineral structure of the earth in many widely separated regions and especially in the Dutch East Indies and South Africa; the Murchison Medal to Ernest Edward Leslie Dixon, of the British Geological Survey, in recognition of the value of his geological researches, especially in South Wales and the North of England; a Lyell Medal to Mrs. Eleanor Mary Reid, for her studies of the fossil floras of the Tertiary and Pleistocene rocks; a Lyell Medal to Professor Leonard Johnston Wills, of the University of Birmingham, for his researches on the stratigraphy and paleontology of the Midlands.

SIR ARTHUR SMITH WOODWARD, until his retirement in 1924 for twenty-three years keeper of the Geological Department of the British Museum, has been elected a member of the Royal Swedish Academy of Sciences in Stockholm in the Section of Zoology.

H. P. CHARLESWORTH, assistant chief engineer of the American Telephone and Telegraph Company, has been reelected chairman of the Engineering Foundation, New York City. D. Robert Yarnall, of the Yarnall-Waring Company, of Philadelphia, has been elected vice-chairman and Alfred D. Flinn has been elected director and secretary. Members of the board of trustees are Mr. Charlesworth, Mr. Yarnall, A. L. J. Queneau, George L. Knight, George Barron, Everette DeGolyer, George E. Beggs, Langdon Pearse, Walter H. Fulweiler, Albert E. White, F. M. Farmer, Walter I. Schlichter, Frederick M. Becket, John V. N. Dorr and Edward R. Fish.

THE tenth national convention of Sigma Gamma Epsilon, professional fraternity for geology, mining, metallurgy, ceramics and petroleum engineers, was held at Hollywood, Calif., on December 27 and 28, 1935. The fraternity now has thirty chapters in all parts of the United States. The following national officers were elected: *President,* Professor E. F. Schramm, University of Nebraska; *Vice-presidents,* Dr. E. P. Henderson, U. S. National Museum, Professor K. K. Landes, University of Kansas, Professor E. T. Hodge, Oregon State College; *Secretary-treasurer,* Professor C. B. Carpenter, Colorado School of Mines; *Historian,* Professor F. M. Bullard, University of Texas, and *Editor,* Professor W. A. Tarr, University of Missouri. The next convention will be held at the University of Texas in December, 1937.

C. G. FISHER, president of the Fisher Scientific Company, has been elected chairman for 1936 of the Pittsburgh Section of the American Chemical Society.

ANNOUNCEMENT is made of the retirement from active service next September of Dr. Reid Hunt, since

1913 professor of pharmacology at the Harvard Medical School.

DR. JOHN A. MILLER, research professor of astronomy at Swarthmore College and director of the Sproul Observatory, has retired.

DR. KARL SAX has been promoted from associate professor of plant cytology to professor of botany at Harvard University. He has been on the Harvard teaching staff since 1928. Previously for eight years he was biologist in charge of plant breeding at the Maine Agricultural Experiment Station.

DR. H. LOWERY, head of the department of pure and applied physics in the College of Technology of the University of Manchester and secretary of the Manchester and District Local Section of the Institute of Physics, has been appointed principal of the North-Western Polytechnic, London.

LOWRY NELSON, of Wisconsin, has been appointed director of the Utah Agricultural Experiment Station at Logan.

AT a recent meeting of the Board of Directors of the Long Island Biological Association it was voted to appoint Dr. Eric Ponder, investigator in general physiology at its Biological Laboratory, interim director, pending the appointment of a successor to the late Dr. Reginald G. Harris.

DR. KARL MURDOCK BOWMAN, of Newton Center, Mass., chief medical officer of the Boston Psychopathic Hospital, has taken over his work as director of the department of psychiatry at Bellevue Hospital, New York City. He succeeds Dr. Menas S. Gregory.

DR. WILLIAM BASIL KEELER, medical inspector for the South Boston health unit, has been appointed health commissioner of Boston, succeeding the late Dr. Francis X. Mahoney.

DR. ULYSSES P. HEDRICK, director of the Experiment Station at Geneva, N. Y., becomes *ex-officio* a member of the Board of Trustees of Cornell University, by virtue of his election as president of the New York State Agricultural Society. He succeeds Dr. Carl E. Ladd, dean of the Colleges of Agriculture and Home Economics.

AT the recent meeting of the International Dermatological Congress, held at Budapest, Hungary, Dr. H. N. Cole, of Western Reserve University, was elected to the permanent committee of eleven on organization, representing the United States.

THE Committee on Scientific Research of the American Medical Association has awarded a grant to Professor Israel S. Kleiner, of the New York Homeopathic Medical College and Flower Hospital, for further work on ascorbic acid, and Assistant Professor Helen C. Coombs, of the department of physiology and biochemistry, has received a grant for work on the action of acetyl choline on the central nervous system.

DR. EDWIN P. ADAMS, Henry professor of physics at Princeton University, has been granted leave of absence for the second term of the current academic year.

JOHN G. BARRY, consulting mining geologist and engineer of El Paso, Texas, has returned to El Paso following an examination trip in Sonora and Sinaloa, Mexico.

UPON the request of the Peruvian Government, the Bureau of Fisheries has loaned it the services of M. C. James, assistant chief of the Division of Fish Culture, for the purpose of making a survey of Lake Titicaca and determining the species of fish best suited to its waters.

DR. JAMES G. NEEDHAM, head of the department of entomology at Cornell University, spoke at the University of Alabama on February 13 on "War—a Biological Phenomenon." The lecture was under the auspices of the Sigma Xi Club and the Research Council. A dinner was given before the lecture at which Professor Needham was the guest of honor.

DR. JOHN JOHNSTON, since 1927 director of research for the United States Steel Corporation, delivered the monthly Sigma Xi lecture at Rensselaer Polytechnic Institute on February 14. He spoke on "The Physical Chemistry of Steel."

DR. R. W. WOOD, professor of physics at the Johns Hopkins University, lectures before the New York University Chapter of Sigma Xi on February 21. His lecture is entitled "Some New Effects Obtained with High Explosives."

DR. JOHN C. HOSTETTER, director of development and research at the Corning Glass Works, gave on February 14 a lecture at the Franklin Institute before the Rittenhouse Astronomical Society of Philadelphia. His lecture, which was preceded by a dinner of the society, was entitled "Glass for the Astronomer from Galileo to the 200-inch Disc." It was illustrated with slides and sound motion pictures.

DR. HENRY E. SIGERIST, William H. Welch professor of the history of medicine of the Johns Hopkins University School of Medicine, lectured recently at Vanderbilt University School of Medicine on "Medicine in the Renaissance," "Medical Organizations in Europe" and "The Life and Work of Louis Pasteur and Robert Koch."

THE following announcements of lecturers for 1936 have been made by the University of Oxford: Sir Donald Tovey, Reid professor of music in the University of Edinburgh, will deliver the Romanes lecture; P. M. S. Blackett, professor of physics at Birkbeck College, will give the Halley lecture, and Emeritus Professor J. A. Smith, the Herbert Spencer lecture.

THE third annual meeting of the American Institute of Nutrition will be held at the Washington Hotel in Washington, D. C., on March 25. The officers for 1935–36 are as follows: *President,* John R. Murlin; *Vice-president,* Eugene F. DuBois; *Treasurer,* George R. Cowgill; *Secretary,* Icie G. Macy; *Members of the council,* Roland M. Bethke, L. A. Maynard and Arthur H. Smith.

THE one hundred and fourth annual meeting of the British Medical Association will be held at the University of Oxford, from July 17 to 24, under the presidency of Sir James W. Barrett, Melbourne, Australia.

BY the will of Roger Deering, Northwestern University receives the sum of $7,000,000. The bequest is unrestricted, and the trustees have determined that the money will not be placed in the building fund but will be used for educational purposes. This bequest brings the total amount received by the university from the Deering family to $10,300,000.

HARVARD UNIVERSITY will receive $100,000 under the will of Miss Belle Hunt, of Boston and Beverly, who died on January 17.

AN Associated Press dispatch reports that the first planetarium in the Far East is to be installed at Osaka, Japan, by the Carl Zeiss firm at Jena, which also has an order for a planetarium at the Paris world exposition in 1937. They are similar to the planetaria in New York, Philadelphia, Chicago and other American cities.

THE British Institution of Mechanical Engineers has set aside the interest on a substantial sum of money for the establishment of a James Watt Medal. It will be awarded biennially for "the most outstanding achievement in mechanical engineering during the previous two years" and will not be confined to British engineers.

THE *Journal* of the American Medical Association reports that Professor Nitescu, as the official representative of Bucharest University, at the International Congress of Physiology recently held at Moscow, while there was invited by the leaders of the Moscow faculty of pharmacy to exchange a certain number of students, beginning next year. Both states will erect a special home in which the exchange students will be given free accommodations.

THE Society for the Preservation of the Fauna of the British Empire, with the approval of the Secretary of State for the Colonies, has asked Sir Thomas Comyn-Platt and Captain Keith Caldwell to visit (1) Ceylon and Malaya, and (2) the West Indies, British Guiana and British Honduras, to report on the preservation of the fauna of those countries, with special reference to the danger of extinction to any rare species. The two missions will be conducted on lines generally similar to those undertaken by Major Hingston to East Africa in 1930, and by Colonel Haywood to West Africa in 1931–32, both of which were made on behalf of the society. Sir Thomas Comyn-Platt and Captain Caldwell planned to leave England early in the year, and will report directly to the society.

MORE than 181,000 visitors—a seven-fold increase since 1929—viewed during 1935 the exhibits in the Free Natural History Museum of the Academy of Natural Sciences, Philadelphia. This is an increase of 18,000 over 1934, and includes some 31,000 school children.

The British Medical Journal calls attention to the condition of the Harvey Memorial Tower at Hempstead Church in Essex. The work of restoration has proceeded, and about two thirds of the structure has been restored; but the fund is now exhausted and the work has ceased. The last third, together with the rehanging of the bells, remains to be completed. A sum of approximately £2,000 is required for this purpose. Lord Horder has consented to become chairman of the committee, in place of the late Sir John Rose Bradford. Donations should be made payable to the Harvey Memorial Fund and sent to Dr. G. de Bec Turtle, Royal College of Physicians, Pall Mall East, London, S.W.1.

A GIFT by Williams College of a 1,700-acre estate to the Federal Government for a forestry experimental station has been announced. The property, given to the college in November, 1933, by Mrs. M. Theresa B. Hopkins, of Boston, widow of Colonel Lawrence Hopkins, was known as Buxton Farms. It was valued at $100,000 a few years ago. The estate, which lies along the Taconic mountain range, makes up the northwestern corner of the Commonwealth of Massachusetts, being bounded on the west by New York and on the north by the Vermont line. The college had found no immediate use for the estate other than for field work by the department of botany and for the uses of the Williams Outing Club. Walter K. Starr, who is connected with the Forest Service Bureau of the U. S. Department of Agriculture, has been making a survey of the property and its resources since last April. A small staff of WPA workers is assisting him. Colonel Hopkins, a graduate of Williams College in the class

of 1863, was a son of the Williams president, Mark Hopkins.

AN 800-ACRE tract of land two miles east of Chapel Hill belonging to the University of North Carolina will be made headquarters of the southeastern states in experimentation of control of soil erosion. Experiments will consist chiefly in growing trees and shrubs for highway bank protection, game conservation and gully control. A 50-foot plot has been prepared for setting out plants to be brought from the government's station at Statesville. E. L. Evinger, horticulturist, graduate of Washington University in St. Louis, is in command of the station; L. S. Haughton, formerly with the Department of Agriculture, will be plant propagater; O. L. Veerhoff, a graduate of the Johns Hopkins University, will make experimental studies in seed germination. The personnel at the station will also include natural scientists who will function as "field men," to collect plants from all over the southeast. Operations on the farm will be steadily expanded. Engineers will be sent from the soil conservation service at High Point to make a topographic survey and a soil survey of the farm. Also, an irrigation system, to cover five acres at first, will be installed. The farm will be financed by the Department of Agriculture but labor will be supplied by the North Carolina division of the Works Progress Administration, a joint enterprise of the government and the university. The project will serve as a laboratory for scientific study and eventually will become an arboretum, containing millions of trees and shrubs.

DISCUSSION

MAGNITUDE AND ENERGY OF EARTHQUAKES

LISTS and catalogues of earthquakes are frequently used for both popular and scientific purposes. Occasionally such lists are compiled without suitable discrimination, including all the shocks, small and large, which may happen to come to notice. Under such circumstances the number of earthquakes listed for any given region is likely to be a better index of the density of population than of the seismicity; for minor shocks are very frequent in all seismic regions, and even in many districts not commonly thought of as seismically active. Similar undiscriminating use of instrumental data gives undue weight to minor shocks occurring near seismological stations.

Some means evidently is required for selecting and listing the larger shocks. The logical procedure is to number shocks on a definite scale, and for general purposes to list only those of higher scale number. Several arbitrary scales are in regular use; these rate shocks in terms of intensity, which properly refers only to the degree of manifestation at a particular point. For example, intensity IV may be defined as that degree of shaking at which windows and dishes rattle, etc. Such a scale does not directly give much information about the earthquake as a whole; given effects may be due to a comparatively small shock originating near by or to a large shock at considerable distance.

When the circumstances of a shock are completely known, a rough rating is supplied by the maximum intensity manifested. However, this maximum varies with the physical conditions of the shock—depth of focus, geological structures, nature of ground, etc.—and the observations will be much affected by accidental circumstances such as the number and character of structures in the shaken area. Moreover, a large majority of strong shocks are submarine or occur in unpopulated areas, so that any reliable listing of strong shocks must depend on seismographic records.

It would be desirable to rate earthquakes in terms of the energy actually liberated in each shock; but at present the determination of such energies from seismograms is subject to large uncertainties. Accordingly, a partly arbitrary magnitude scale has been set up,[1] originally for use with local shocks of the Southern California region. The magnitude of a shock is defined as the logarithm of the amplitude written by a standard seismograph distant 100 km. from the epicenter. Thus a shock of magnitude 7 writes amplitudes 10 times as large as those for magnitude 6, 100 times those for magnitude 5, etc.; the corresponding energy ratios should be 100 and 10,000. The arbitrary zero of the scale has been chosen to coincide with the smallest recorded shocks. The smallest shocks reported felt are of magnitude 1.5, the smallest causing any damage are about 4.5, and major earthquakes exceed magnitude 7, but in most cases the intensity is distributed so irregularly that it can not be used to determine the magnitude.

This scale has now[2] been extended, with some additional uncertainty, to apply to large shocks in all parts of the world, the data being the earth motions recorded at a considerable number of stations. The largest shocks, beginning with 1904, have been investigated in this way. The selection is not difficult, as the greatest shocks produce surface waves with amplitudes of a millimeter or more over nearly the whole earth. The

[1] C. F. Richter, *Bull. Seism. Soc. Amer.*, 25: 1, 1935.
[2] B. Gutenberg and C. F. Richter, *Gerlands Beiträge zur Geophysik*. In press.

period of this large motion is about 20 seconds, so that it is not perceptible.

The resulting list includes 49 shocks with assigned magnitudes over 7½. Four of these are of magnitude near 8½; all the evidence indicates that these are really exceptionally large. The dates and places are: January 31, 1906, Ecuador and Colombia; January 3, 1911, Turkestan; December 16, 1920, Kansu (China); November 11, 1922, Atacama (Chile). Six others are of magnitude about 8¼. April 18, 1906, San Francisco; August 17, 1906, Valparaiso (Chile); June 26, 1917, Tonga region; February 3, 1923, off Kamchatka; March 2, 1933, off Japan; January 15, 1934, India. Notwithstanding its apparent high intensity, the earthquake destructive at Tokio on September 1, 1923, was somewhat smaller than these. The remaining shocks of the list range from about 7¾ to 8. The most recent shock listed is the Quetta (Baluchistan) earthquake of May 30, 1935 (about 7¾).

A number of well-known shocks are not included—such as the disastrous Messina earthquake of 1908 (magnitude about 7). The California earthquake of 1906 is the only listed shock occurring in the United States. (Of course, other large shocks have occurred in this country prior to 1904.) It may be of interest to give here the magnitudes of a few of the larger shocks of recent years in the continental United States.

Montana	June 28, 1925	6¾
Santa Barbara, Calif	June 29, 1925	6¼
Texas	Aug. 16, 1931	6½
Nevada	Dec. 20, 1932	7½
Long Beach, Calif.	Mar. 10, 1933	6¼
Utah	Mar. 12, 1934	6¾
Helena, Montana	Oct. 18, 1935	6½ ±
Helena, Montana	Oct. 31, 1935	6½ ±
Southeastern Canada	Nov. 1, 1935	7 ±

This list is in no sense complete. The last three values are tentative only. Special attention is directed to the magnitudes of the Santa Barbara and Long Beach earthquakes, as the notion still persists that these shocks, especially the latter, were great shocks of the type of the San Francisco earthquake. In point of fact the energy released in that case was probably of the order of 10,000 times that of the Long Beach earthquake. The spectacular damage and considerable loss of life in the smaller shock are chiefly to be attributed to the failure of weak structures on unstable ground.

The total of 49 great shocks in 32 years should not be taken too hastily as a measure of the seismicity of the globe. The magnitudes are calculated from the amplitudes of surface waves; this procedure applies only to shocks at normal depth. Many shocks occur at abnormally great depths; these give rise to very small surface waves or none at all, although in some cases their energy may possibly equal that of the greatest normal shocks.

Very divergent statements as to the frequency of earthquakes, for the whole earth or for particular regions, will be found in the literature. A magnitude scale or its equivalent is a necessary preliminary to reliable statistics of this character. As a preliminary sample of the sort of conclusions to be expected from investigations now under way, we offer the following results.

The occurrence of ten shocks of magnitudes 8¼ to 8½ in 32 years is a somewhat questionable index of the frequency of very large shocks, as we can not be certain that the period 1904–1935 is sufficiently long to represent the normal conditions. For the shocks of magnitude approximately 8 we find an average of roughly one a year. To arrive at estimates for the smaller magnitudes, the reports for 1926 have been examined with some care. In that year there were no shocks of magnitude 8 or over, but there was one shock of magnitude about 7½, and seven others of magnitude 7 or slightly higher. About 350 shocks were instrumentally registered over at least half the surface of the earth; this corresponds roughly to a magnitude of 6 and over. For smaller magnitudes no exact figures can be given for the entire earth, as there are large areas in which small shocks might occur without being registered at the nearest stations. The fact that about 5,000 shocks were included in the International Summary (Oxford) for 1926, which digests all available data from the stations of the world, indicates that shocks of small magnitude are very frequent. This is borne out by the results obtained at Pasadena. During 1934 a total of 213 shocks of magnitude 3 and over originated in a selected area including Southern California and a small part of adjacent Mexico. Of these 114 were of magnitude 3, 63 of magnitude 3.5, while the highest magnitudes were 5.5 and 6, represented by only one shock each. (Magnitudes are assigned in routine to half a unit.) If these ratios are generally applicable, there must be between 10,000 and 100,000 shocks of magnitude 3 annually over the whole earth. Such shocks are usually reported felt generally near the epicenter. Statistics of this kind would not include the thousands of very small aftershocks which follow each major earthquake.

As pointed out above, the relation between the assigned magnitudes and the shock energies is not known with precision. Several methods have been used to estimate the total energy liberated in the largest shocks; the results agree reasonably well, the most probable value at present appearing to be 10^{25} ergs (for shocks of magnitude about 8½). The corresponding value for the very smallest shocks registered is

about 10^8 ergs. This latter energy is of course very small; but shocks of this magnitude are registered only at very short distances, and then only by the most sensitive instruments, on whose records they are barely perceptible above the usual ground unrest. Calculation using the physical constants of the instruments indicates that this should be the case for shocks in which an energy of 10^8 ergs is liberated in a very small time. Since the ratio of the largest to the smallest shocks is determined from the observations, it follows that the estimated energy for neither group can be seriously in error (probably by not more than a factor 10). This tends to remove doubts, such as have frequently been expressed, as to whether energies so large as 10^{25} ergs actually are liberated in earthquakes.

In the first paper on the magnitude scale (*loc. cit.*) it was pointed out that the seismic energy liberated in the California region during a given period is almost wholly accounted for by the larger shocks; smaller shocks are not sufficiently frequent to contribute more than a small fraction of this energy. A similar result is indicated, with slightly less definiteness, for the large shocks of the world. During the last 32 years the listed shocks of magnitude approximately 8 or over represent an estimated energy of about 10^{26} ergs, or an average of 3×10^{24} ergs annually; while in 1926, during which year none of these larger shocks took place, the total liberated energy was of the order of 2×10^{23} ergs. Very small shocks do not need to be considered; 100,000 shocks of magnitude 3 would give only 10^{19} ergs. Such evidence strongly indicates that the smaller shocks do not appreciably mitigate the strains which are released in the larger earthquakes, but must be regarded as minor incidents in and symptoms of the accumulation of such strains.

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THE DEATH OF HUNDREDS OF CEDAR-WAXWINGS[1]

THE cedar-waxwings, *Bombycilla cedrorum* (*Ampelis cedrorum*), are chiefly distinguished by the bright red wax-like appendages on the secondary coverts of the wing feathers, although they have many other remarkable characteristics.

These birds feed on insects and small wild berries, scarcely ever touching cultivated fruits.

There is no noisy warning of the coming or going of a flock of "cedar-waxwings." They are very sociable and fly in flocks of very close formation while in search of food. A group of two to five hundred or more keep so closely in contact with each other that they act as if of one mind. Sometimes apparently bent upon going in a definite direction, they suddenly turn about and settle en masse on some tree or bushes, quite noiselessly, having either scented or seen the food as they flew over.

A visitation of a flock of "waxwings" to an apple orchard is welcomed, because it means less worms and better apples the next year. It is very unfortunate to have a flock of these beautiful useful birds destroyed.

On March 19, 1935, a cold morning after the rains, a rather large number of these birds entered Los Angeles, settled on some ornamental Canary Island date palms (*Phoenix canariensis*) and began feeding on the water-soaked dates. The delicious odor and the sweetness of the fruit proved to be a fatal lure; for shortly after they had eaten of the dates the birds began to fall all about dead or dying in one to ten minutes of asphyxia or paralysis-like symptoms. Some recovered after a longer time and flew away. It was noticed that most of the fatalities occurred about one tree. Some of the birds were obtained the next day and autopsies were performed.

In each bird the post-mortem conditions were the same. At the base of the skull of the posterior portion of the cranium, between the supra-occipital and the atlas, in the region of the foramen magnum, there was a large accumulation of blood. The lungs and right side of the heart were full of blood, and the liver and kidneys congested. The blood in general was rather light red in color.

Pieces of the outside of the dates, meat and hull were found in the digestive tract. Pieces as large as half a date were found in the crop, showing the birds had fed very greedily. The birds were in good condition, plump and fat. No parasites were discovered, and no evidence of infection with microorganisms was found, also no indications of metallic poisoning. No pathology was noted.

The dates were bruised and water-soaked. The outer covering remained mostly intact, while the inside consisted of a watery mushy pulp.

The syndrome, or picture, obtained from the symptoms and post-mortem findings gave evidence of hydrocyanic poisoning.

Conclusions were that the prolonged cold rains and bruising of the fruit by the whipping of the winds, had destroyed the protoplasmic structure of the date

[1] Presented at the meeting of the Pacific Coast Division of the American Association for the Advancement of Science, June 27, 1935.

tissue, permitting the enzymes to hydrolize a glucosoid in the fruits, liberating the hydrocyanic acid.

Due to the low temperature of that morning, sufficient amounts of this extremely toxic compound had accumulated in the fruits to cause the death of the cedar-waxwings.

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A NEW LOCALITY FOR THE BLACK WIDOW SPIDER

AN adult female of *Latrodectus mactans* (Fabr.) was found early in July, 1935, at Parfrey's Glen, near Prairie du Sac, at the edge of the Driftless Area of Wisconsin. This is the first case of finding this spider within the boundaries of this state.[1] The web, found in tall marsh grass about two and a half feet from the ground close to a marshy plot near a small stream, was of characteristic construction. The spider was brought back to the laboratory, where she laid two eggs sacs within five days of each other. The young hatched in about twenty days.

It is of particular interest to note that two males and two females of *Gea heptagon* (Hentz), an orb-weaver previously reported only from the southern United States and as far north as the District of Columbia,[2] were also found in the same locality. A collection of spiders made during the past four years, including about four thousand specimens and over two hundred species from many different localities throughout Wisconsin, has failed to reveal the presence of these two species elsewhere in the state.

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FRESH-WATER MEDUSAE IN IOWA

ON August 20, a strange animal was reported in Avon Lake, a gravel pit 10 miles southeast of Des Moines. On investigation many specimens of the fresh-water medusae, *Craspedacusta sowerbyi*, were found moving about near the surface of 15-foot water.

At different times, these animals were collected and kept alive in jars for ten-day periods. The character of 4 tentacles erect while swimming could not be definitely ascribed to this species. Some individuals did show a tendency to hold the long tentacles upward, however the majority of medusae observed held the tentacles in a pendulous position.

Observations were kept on these coelenterates in their natural medium for 30 days. At the end of this period they had moved out of the deeper water into shallower reaches. Specimens on the 18th of September swam, either with all tentacles upward or all held downward.

This is the first time apparently that *C. sowerbyi* has been recognized in Iowa.

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SCIENTIFIC BOOKS

VAGARIES OF BELIEF

Wish and Wisdom: Episodes in the Vagaries of Belief. By JOSEPH JASTROW. D. Appleton-Century Company, Inc., New York, 1935, 394 pp., $3.50.

MORE than three hundred years ago Francis Bacon, having discerned that knowledge can be power, set himself the task of describing and classifying the knowledge which is not power. He divided it into three groups: the delicate, the contentious and the fantastic. Like the knowledge which is power, fantastic knowledge is an effort to discriminate and to manipulate natural causes in such a way as to control the march of natural events. Like true knowledge, this effort takes on a certain pattern of procedure and operation. But unlike true knowledge of causes this effort neglects the conditions and ignores the methods of control. In effect, it deliberately leads human beings astray.

In this forthright and epigrammatic book Joseph Jastrow resumes Bacon's epic theme. He brings together samples of fantastic knowledge from all the ages, beginning with Lucian's classic Alexander and stopping with Richet and von Reichenbach and their contemporaries. He stops, he does not finish, because the material goes on, not only reproducing old fantasies but generating new ones. It can be exhibited by the method of sampling alone. That the analysis and judgment of the samples should carry conviction to believers in the fantastic is not, of course, to be expected. The study can only confirm those who already agree with Mr. Jastrow in their infidelity: they can not convert the true believers. But to his fellows in infidelity Mr. Jastrow's book should bring merriment and illumination. Between a "Foreword" and an "Afterword" of psychological comment, he sets, decently and in order, a chain of instances which he classifies as "Credulity," "Magic and Marvel," "Transcendence," "Prepossession," "Congenial Conclusions," "Cults and Vagaries" and "Rationalization." Under "Credulity" he links the picaresque career of

[1] C. E. Burt, *Jour. Kans. Ent. Soc.*, 8 (4): 117–130, 1935.

[2] A. Petrunkevitch, *Bull. Am. Mus. Nat. Hist.*, 29: 345, 1911.

"Leo Taxil" with Conan Doyle's belief in fairies. Under "Magic and Marvel" he groups ideas about the devil and the not-so-strange case of Madame Blavatsky. Under "Transcendence" he places the various marvels that psychical researchers research into. Under "Prepossession" figure believers in that Aryan genius, the horse *Kluge Hans,* in clairvoyants and similar instruments of the will-to-believe. Under "Congenial Conclusions" the mysteries of palmistry and numerologies are revealed. Under "Cults and Vagaries," Mr. Jastrow expounds speculations, from phrenology to eccentric theories of various scientists and pseudoscientists. Under "Rationalization" he pays disconcerting attention to "od," "n-rays," auras, vibrations and ectoplasms.

The tale of these adventures of the mind is, as Mr. Jastrow tells it, a picaresque epic, pointing a rationalist moral, and told with the incisiveness and epigrammatic pith which distinguishes Jastrow's psychological writing from most of his colleagues'. From his standpoint of classical materialistic determinism, he designates these systems of belief as "pseudologies" or "thobbings," and, if I understand him correctly, attributes them to the event that "the logical occupation is beset by intrusions from other human interests." They are to him waves of the continuous flow of belief through folklore and doctrine: there survive in them ancestral wish and traditional wisdom whose root is subjectivism and whose heart is the inveterate habit of jungle magic. He regards the various categories of wish as the forms of intrusion upon "the logical occupation." He thinks that the intrusions can occur because the thought involved is essentially vague, implemented by analogy and symbol and resting on a feeble sense for fact.

All of which is undoubtedly correct. But it is also correct that Mr. Jastrow's judgment upon the vagaries of belief is made retrospectively and externally; that it derives from a set of premises the believers do not share, and that these premises are themselves no less objects of faith than the propositions they contradict. The "scientific" concepts which he employs as his standard of judgment seem to me to differ only in content and consequence, not in mood and method from the vagarious concepts he judges. Analogy and symbol are as inseparable from science as from faith, a fantasy can be as articulate and precise as a fact, a scientific notion as vague as a fantastic one. Neither logical structure nor laboratory procedure can convincingly validate our beliefs; only developing consequences can do that. As Mr. Jastrow himself observes, the history of science is a history of the outgrowth of error. But what makes any belief an error is not its intrinsic character but its displacement by another belief which enables a better control of the same field. "Error" is a retrospective judgment of cognitive value. Like truth, it has no intrinsic criteria. If it had, nobody could any longer be a Christian or a Mohammedan or a Judaist, to say nothing of a vagarist in any other field. Mr. Jastrow's deliverances seem harsher and less tolerant than his excellent demonstration of his case requires.

H. M. KALLEN

NEW SCHOOL FOR SOCIAL RESEARCH

THE STRENGTH OF MATERIALS

Elements of the Strength of Materials. By S. TIMOSHENKO and GLEASON H. MAC CULLOUGH. Cloth; 6 × 9 in.; pp. 350. Line drawings and tables. Published by D. Van Nostrand Company, Inc., New York City. $3.25.

ENGINEERING text-books on strength of materials may be roughly divided into three classes: (1) drill books which develop the simple formulas used by structural engineers and machine designers and which emphasize the immediate practical application of the formulas to the commoner units of design—beams, shafts and columns; (2) books which emphasize the mathematical development of formulas, which take up more elaborate analyses than do the books of the first class, and which, in their advanced chapters at least, approach the outer courts of the mathematical theory of elasticity, and (3) text-books which emphasize the structure and mechanical properties of actual materials of construction, as well as the ordinary formulas for stress analysis of parts assumed to be made of the ideal homogeneous and isotropic material assumed in the mathematical theory of elasticity.

This book is an excellent example of the second class. As might be expected (and welcomed) from the name of the senior author, it has a distinct European flavor in its symbols, its nomenclature and its general approach to problems. In saying that this book emphasizes mathematical development of stress analysis the reviewer does not intend to convey the idea that it is one-sided. It contains a chapter on the properties of materials with a brief discussion of theories of failure, and a short, but interesting, discussion of working stresses.

Two features of the book strike the reviewer as especially worthy of study—the treatment of beams made of material which does not follow Hooke's law and the section on yielding and buckling of columns. An excellent mechanical feature of the book is the arrangement in self-contained sections of alternate methods of mathematical treatment of the deflection of beams, and of stress analysis of statically indeterminate beams, so that, if lack of time makes it necessary for the teacher to omit the study of any particular method, it can be done without interfering with the

study of other methods. Special subjects treated in the book include curved beams, eccentric loads on columns and a study of energy of strain with special application to determination of stress under impact.

The book is a worthy addition to the text-books on the mechanics of materials and is especially recommended to the attention of teachers and scholars who wish to emphasize the mathematical development of stress analysis in their beginning courses in strength of materials. It would make an excellent book for the use of "honor sections" of students of outstanding ability.

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SPECIAL ARTICLES

THE AGGREGATION OF ORTHIC TETRAKAIDECAHEDRA

A MODIFICATION of the regular octahedron in combination with a cube, a figure with fourteen sides, eight hexagonal and six square, all the edges of all the hexagons and of all the squares being equal, was well known to the crystallographers of the eighteenth century. This figure was called the tetrakaidecahedron by Lord Kelvin[1] in a theoretical essay "on the division of space with minimum partitional area." In recent years, with increased emphasis on problems of morphogenesis, this figure has come to be of renewed interest.

The biological significance of Kelvin's figure has been demonstrated by Lewis,[2] who has shown the tetrakaidecahedron to be a fundamental shape for both plant and animal cells (pith cells of elder and rush, epidermal cells of cucumber and tradescantia, human oral epithelial and adipose tissue and precartilage cells from the toad), when the cells are aggregated into tissues.

The shape of cells may be the result of various factors, such as surface tension phenomena, the law of bipartition, contact and pressure, cell and tissue differentiation and possibly organ configuration. Among the fewer-celled organisms especially, the principle of bipartition is obviously of great significance. And in the higher plants the shape and the arrangement of the cells may well be influenced, in a measure at least, by the adjustments that result from the operation of the law of bipartition in its relation to the ultimate stacking of the tetrakaidecahedra.

It is obvious that fourteen orthic tetrakaidecahedra can be stacked around a central one to produce an aggregate of fifteen. Thus the first layer contains fourteen members surrounding a central one. By stacking paper models of the type described by Matzke,[3] it was found that 50 tetrakaidecahedra can be stacked around these fourteen. The second layer has 50 members, making an aggregate of 65 for the whole mass. Similarly, it was determined that 110 tetrakaidecahedra can be stacked around these 50. The third layer accordingly has 110 members and the aggregate is 175; the fourth layer has 194 members and totals 369. From these data two formulae may be derived, one for the number of members in any given layer, and the other for the total number of members in the aggregate at any given layer. If T_n denotes the number of members in any layer where n represents the number of the layer, then $T_n = 12\ n^2 + 2$. If S_n equals the total number of members in the aggregate at any layer where n is the number of the layer, then $S_n = 4\ n^3 + 6\ n^2 + 4\ n + 1$. Thus for example if $n = 2$, then $T_n = 50$, and $S_n = 65$. Table I gives the aggregate per layer for some of the layers in the tetrakaidecahedron series and also gives the bipartition series.

TABLE I

	Tetrakaidecahedron series	Bipartition series
Layer no.	Aggregate per layer	Aggregate
1	15	2
2	65	4
3	175	8
4	369	16
5	671	32
6	1105	64
7	1695	128
8	2465	256
9	3439	512
10	4641	1024
11	6095	2048
12	7825	4096
13	9855	8192
14	12209	16384
15	14911	32768
16	17985	65536
17	21455	131072
18	25345	262144

It is apparent that bipartition and economy of surface relations (the latter illustrated by stacking tetrakaidecahedra) result in series of cells that do not aggregate at the same rate, but converge at first and diverge afterward. Attention is called to numbers in one series which are approximated by a number in the other, such as 15–16 and 65–64. The failure of these series to coincide may well have morphogenetic significance—for instance, in the establishment of polarity—in the development of the organism from the unicellular egg to the multicellular adult.

Additional data and the proof of the formulae given above will be presented in a subsequent publication.

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[1] Lord Kelvin, *Phil. Mag.*, 5s. 24: 503–514, 1887.
[2] F. T. Lewis, *Proc. Amer. Acad. Arts and Sci.*, 58: 537–552, 1923.
[3] E. B. Matzke, *Torreya*, 31: 129–136, 1931.

SUCCESSION OF COLI-AEROGENES ORGANISMS IN THE HEALTHY ADULT FECAL FLORA

WE[1] have called attention to the succession of coli-aerogenes forms which takes place when normal human feces are suspended in saline and held at 37° C. and in the ice box. Storage effects a complete turnover in the flora of this group, the predominant Escherichia (coli group) of fresh feces giving way, stage by stage, to an ultimately lactose-degraded Aerobacter (aerogenes-cloacae group) flora in a few weeks time.[2] Exceptions to this generalization occur whenever the original freshly excreted specimen contains (as far as this bacterial group is concerned) only coli. Such exceptions, which are not rare, lend support to the contention that the changes in flora observed represent succession rather than variation.

Further work reveals that the coli-aerogenes flora of fresh feces from healthy adult humans may present such deviation from the accepted normal that it is probable this group maintains its recognized distribution by a balance so delicate that it may occasionally be much disturbed or the organisms even temporarily eliminated. The coli-aerogenes group in fresh feces is ordinarily represented by more than 90 per cent. of coli and less than 10 per cent. of aerogenes with an occasional coli-aerogenes intermediate (Citrobacter) appearing. Frequently specimens are encountered from which only coli can be grown. On the other hand, samples are met with in which the flora of this group is largely or entirely citrate utilizing (Aerobacter and/or Citrobacter). And there are those from which no organisms of the group may be isolated either by direct plating or by plating after preliminary enrichment in lactose broth.

Illustrative cases will make the point clear and point to its significance. Fifty-seven fresh fecal specimens deriving from "A" at intervals between March 21, 1933, and April 23, 1935, were examined and 661 strains carefully studied. In addition several hundred other strains were studied only for their ability to utilize citrate as their sole carbon source. Direct platings were made on Endo's medium from the fecal suspensions and lactose broth enrichment tubes seeded. These last were only utilized in case no growth took place on Endo's medium. Strains for the complete study were after careful purification tested for their dissimilation of dextrose, lactose, saccharose, dulcite, salicin, alpha-methyl-d-glucoside and cellobiose; for the production of acetyl-methyl-carbinol, indol and hydrogen sulfide; for their action on milk and gelatin, for their methyl red reaction; and for their ability to utilize citrate as the sole carbon source. The coli-aerogenes flora of this subject was on 51 occasions predominatingly coli (96.3 per cent.).

However, from his specimen 45 of December 28, 1934, no organisms of the group could be isolated, even utilizing enrichment methods. Five days later a sample showed only citrate utilizing, hydrogen-sulfide producing coli-aerogenes intermediates (50 studied). This is a form rare in the usual flora. On January 7, 1935, specimen 47 was analyzed. Of 20 colonies studied, 17 were aerogenes and intermediates, 3 coli. Two days later a sample yielded no organisms of the group on adequate direct plating. By enrichment procedures coli was obtained. Three days later no organisms could be obtained directly from specimen 49, but by enrichment aerogenes and intermediates were recovered. On January 13, 1935, the fiftieth specimen examined for this subject again yielded no group organisms on direct plating but a sparse yield of coli was obtained via enrichment. Two days later and on six subsequent occasions specimens were analyzed and at each analysis organisms were readily obtained by direct plating. Seventy of 72 strains intensively studied were coli and of 161 additional strains tested on citrate only all failed to develop (coli).

Again, the "C. Y." specimen of February 6, 1935, yielded 29 citrate utilizers of 52 strains examined, but his specimen of February 28 was 100 per cent. coli—35 colonies fished and studied. The "D. K." sample of March 25, 1935, which yielded 80 per cent. citrate utilizing coli-aerogenes, was followed in our series by a 100 per cent. non-citrate utilizing flora for the specimen of April 13. Lastly, "E. B. C." submitted fecal specimens on March 5 and 15, 1935. From neither could coli-aerogenes organisms be isolated, even by enrichment procedures. The third specimen, submitted on April 23, was, however, entirely coli—62 colonies studied. All subjects were in good health and on average mixed diet with the possible exception of "E. B. C.," who was dieting. It should be noted that this diet, to avoid weight, was no more rigorous than very many of our population subject themselves to and in view of the prevalence of such procedures if the diet had anything to do with the results obtained it only makes the point more significant.

The importance of these findings, chosen from 100 rather complete analyses of fresh fecal specimens from 31 different persons, should be apparent. It is possible that in any contamination of water or food stuffs from community excreta the generally accepted coli aerogenes picture may be valid. When, however, one considers contamination from a single source it is

[1] L. W. Parr, *Proc. Soc. Exp. Biol. and Med.*, 32: 580, 1935.

[2] L. W. Parr, *Proc. Soc. Exp. Biol. and Med.*, 31: 1019, 1934.

entirely possible for gross fecal pollution to be present in the absence of recoverable colon bacilli or even of any members of the coli-aerogenes group. This fact may explain certain explosive outbreaks of water-borne disease, such as have occasionally been reported —outbreaks with gastro-intestinal symptomatology and probable etiology but unsolved as to microbial causation. These data explain discrepancies in existing data on the coli-aerogenes distribution in normal feces. Changes in flora of the type reported have been noted, though rarely, in pathological conditions. That such change may occur in normal subjects has not been emphasized and renders less significant such reports in cases of disease. These findings make it increasingly clear that there is no possibility, as yet, of distinguishing between fecal and non-fecal forms of the coli-aerogenes group in any specific instance, despite the probability that in general non-citrate utilizing coli are typically fecal and the other forms of the group less so. If a presumably healthy individual may for days pass feces from which the bacillus nominated as the fecal indicator par excellence can not be recovered one may ask whether there is not still work to do in sanitary bacteriology. If the coli-aerogenes group is our best indicator of intestinal contamination the need for rigid control of water and food processing is emphasized from a new angle. Not the least interesting of the points raised is the question of the mechanism whereby the lactose-fermenting, gram-negative, non-sporing aerobes of the bowel are so grossly disturbed in their relationships or indeed temporarily eliminated in the apparently healthy adult. Bacterial antagonisms and inhibitions, bacteriophage activity, and sub-clinical infections involving disturbances of bowel conditions are a few only of the points which may well be looked into in this connection.

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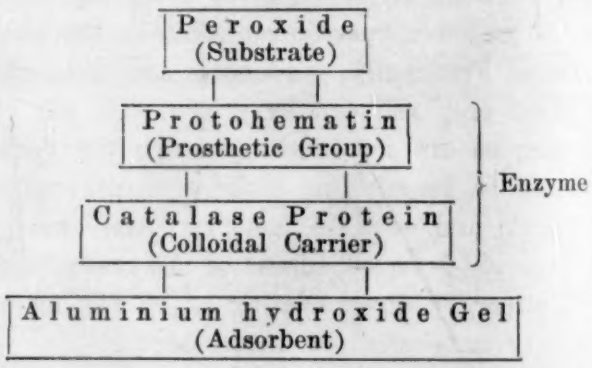
MODE OF COMBINATION OF AN ENZYME WITH AN ADSORBENT AND WITH A SUBSTRATE

IF a purified and highly active solution of liver catalase[1] is adjusted to pH 5 and stirred with a suitable amount of aluminium hydroxide gel or of silicic acid, a large fraction of the enzyme is deposited on the adsorbent. The adsorbate is washed with and suspended in distilled water. When placed in an optical trough of 1 cm thickness and examined in the condensed beam of a 500 watt projection lamp by means of a pocket spectroscope, a three-banded absorption spectrum is observed which proves to be exactly the same as that of the enzyme in solution.[2] The adsorbate is catalytically active towards hydrogen peroxide and monoethyl hydrogen peroxide. With the latter substrate, the two-banded spectrum of the intermediate enzyme-substrate compound and the whole reversible spectral cycle, as reported for the dissolved enzyme,[3] may be seen. Likewise, cyanide will combine with the hematin grouping of the enzyme in the adsorbed state.

Inasmuch as it is known that invertase is eluted from an adsorbate by its substrates, it was necessary to ascertain whether the catalytic activity of the adsorbate in the present case is not due to a preceding desorption of catalase by the peroxide. However, when an adsorbate suspension to which an excess of monoethyl hydrogen peroxide had been added was subjected to filtration while the enzymatic reaction was in progress, a colorless, catalase-free filtrate was obtained, proving that the catalysis and the spectral changes occur directly on the surface of the adsorbate.

The extent of adsorption of the enzyme depends on the type of adsorbent used. Kaolin, finely powdered quartz and activated charcoal did not give satisfactory results under similar experimental conditions.

The enzyme catalase consists of protohematin[4] and of a specific protein.[5] During the heterogeneous catalysis the following arrangement exists:



Free hematin or pyridine-parahematin will not form compounds with peroxides of the type obtained in the case of catalase or methemoglobin. Considering the much smaller activity of methemoglobin compared with that of the enzyme, it follows that not only is a protein component apparently necessary for the formation of the intermediate compound but also the specific nature of the protein determines the decomposition rate of the intermediate. The protein may provide for proper spacing of the hematin groups on the enzyme surface.

It is suggestive to depict the enzyme-substrate compound in the manner preferred by Haurowitz[6] for the

[1] Prepared according to Zeile and Hellstroem.[2] The monomolecular velocity constant of the solution used for these experiments was k = 4525.

[2] K. Zeile and H. Hellstroem, *Z. physiol. Chem.*, 192: 171, 1930.

[3] K. G. Stern, *Nature*, 136: 335, 1935.

[4] K. G. Stern, *Nature*, 136: 302, 1935. *J. Biol. Chem.*, 112: 661, 1936.

[5] K. G. Stern, *Z. physiol. Chem.*, 208: 86, 1932; 217: 237, 1933.

[6] F. Haurowitz, *Z. physiol. Chem.*, 232: 159, 1935.

hydrogen peroxide-methemoglobin, namely as a co-ordinative linkage between the porphyrin-bound trivalent iron and the peroxide molecule. The stoichiometry in the two cases may, however, differ.[3] In contrast to methemoglobin, catalase will combine with the substrate at a pH where both, at least in part, exist as anions. This may be of some significance in explaining the behavior of the two catalysts.

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DECEMBER 14, 1935

SCIENTIFIC APPARATUS AND LABORATORY METHODS

PIPETTING DEVICE FOR DISINFECTANT TESTING

IN the determination of phenol coefficients by the F.D.A. method, it is necessary to inoculate suspensions of pathogenic microorganisms into medication tubes with quantitative accuracy. Pipettes are the prescribed equipment for measuring the dosage of culture, which must be introduced without the tip of the pipette touching the disinfectant. The tip may be allowed to rest against the side of the tube just above the surface of the liquid. It is obvious that greater precision will be obtained if uniform and accurately measured samples of culture are used and if organisms are not left on the sides of the tube where they might escape adequate contact with the solution.

We have found that a simple device regulates the spatial relations of pipette and test fluid in the medication tube, irrespective of the fatigue of the worker, and permits an unusual precision of technique in this test, which in our experience greatly reduces irregularities of results. Furthermore, our arrangement reduces the danger of infection of the technician or of the laboratory.

Fig. 1 shows the device, assembled for use. It consists of a graduated pipette A, actuated through suitable tubing by a heavily greased, 10 ml syringe B. An essential feature is a 90° V-shaped guide trough C, fixed in a vertical position to a convenient support. We have used a supporting block of wood fastened to a ring stand with ordinary burette clamps D. This trough is conveniently formed from No. 18 gauge aluminum, 2″ × 12″, with its long edges rolled back. (This size is useful for medication test-tubes, 25 × 150 mm). A screw clamp on one edge of the guide trough serves as a stop E to fix the position of the test-tube in use. The spring clamp F, attached by clamp fasteners G and short pieces of steel rod to the ring stand, is adjusted to hold the pipette in a position equidistant from and parallel to the walls of a test-tube H held against the trough. A beaker I directly below the pipette and containing a small volume of 1:1000 $HgCl_2$ serves as a trap for any accidental dripping of culture.

When a test is to be made, the guide trough is

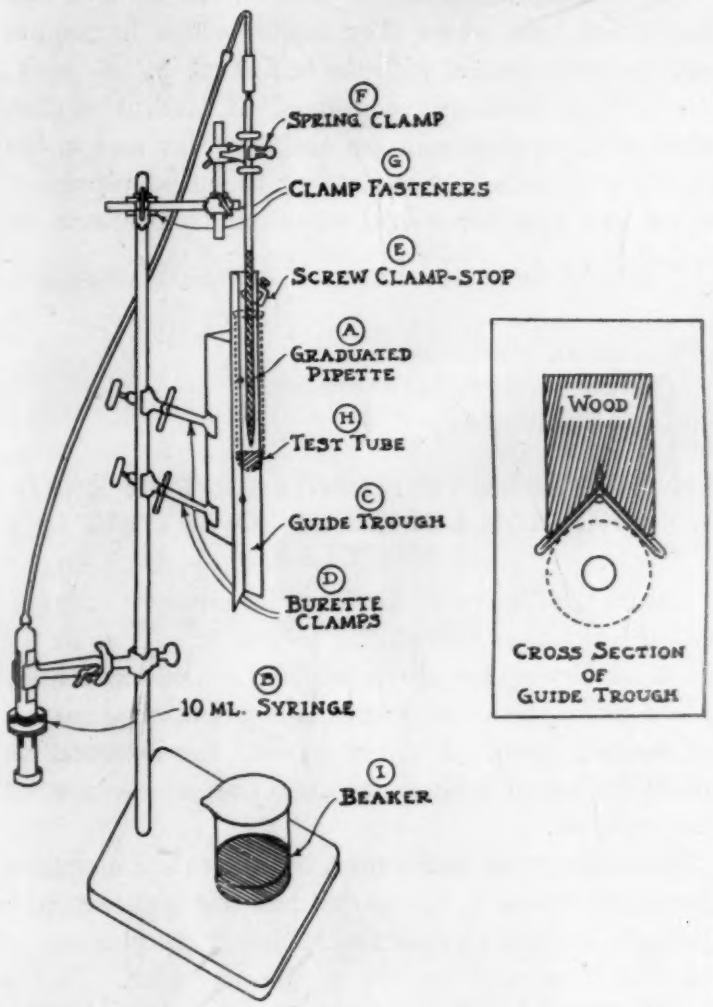

FIG. 1

flamed with a bunsen, the free end of the rubber tubing is slipped over the mouth end of a sterile, cotton-plugged pipette which is then clamped into position. A test-tube of bacterial culture is slid along the trough to a point where the pipette is just above the sediment, and the pipette is then charged with inoculum by withdrawing the syringe plunger. Inoculation is effected into a medication tube slipped into place where the level of its contents will be substantially 1 cm below the pipette tip. By means of the syringe the desired volume of inoculum is dispensed smoothly. The last drop may be withdrawn or dislodged, provided the same technique is always followed.

This device has the following advantages:

(1) A uniform suspension, avoiding sediment, may be selected, since the whole pipette is in full view of the operator, instead of in distorted myopic perspective.

(2) There can be neither infection *per os* of a careless or fatigued technician nor spattering of drops on floor or bench top.

(3) Uniform delivery of uniform dosage is made directly into the disinfectant solution without spattering and without loss on sides of test-tubes.

(4) No organisms can be left on the sides of the medication tube where they might escape immediate and complete contact with the test solution.

(5) Time ordinarily consumed in needful caution when using a pipette in the ordinary way and in the multiple motions for positioning tube and pipette is made available for useful attention to precision of technique.

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AN INEXPENSIVE MAGNIFIER FOR READING FILM COPIES OF SCIENTIFIC ARTICLES

SINCE the library of the U. S. Department of Agriculture has been distributing copies of papers in the form of film copies, there has been considerable ingenuity shown in various institutions to find some method of reading them. A recent paper,[1] for instance, reports the use of a binocular dissecting microscope for this purpose.

It is now pretty well known that there is a magnifier for this purpose on the market and that a special projector is being developed for rendering the films easier to read than with the use of the magnifier. To the writer it seems as if the ideal method for any institution to arrange for reading these films received by its members would be to have a projector in some central place, designed for use in careful study, and a magnifier intended for more superficial perusal available to each member of the staff who would be likely to receive articles sent out in this form. The present cost of the magnifiers, however, might make this out of the question in some institutions.

For this reason it was with much interest that a still cheaper magnifier already on the market (price $1) was recently observed. This magnifier is ordinarily sold as a sort of toy and designed for looking with both eyes at stereoscopic views of pictures put out by the company manufacturing the magnifier. The magnifier is sold under the trade name "Tru-vue" and is a product of the Rock Island Bridge and Iron Works, Rock Island, Illinois. The films that the company supplies for use with this magnifier are standard 35 mm cinema films, the same as used for the film-stat reproductions. Accordingly, the latter can be used in this "Tru-vue" magnifier by simply blocking up one eyehole in it so that one is not looking at one page with one eye and at another page with the other.

The magnification thus secured is less than with the magnifier designed especially for reading these films, and no focusing is possible. The "Tru-vue" magnifier is set in focus for use with normal eyes, and as a result a far-sighted or near-sighted person can not use it except with his glasses in addition. The writer has had a number of different individuals try it, however, and although some have experienced more difficulty than others in reading the text, no one has proved unable to do so if he also used properly fitted glasses and had sufficient illumination.

Although this magnifier is not as perfect, therefore, as the one put on the market for this particular purpose, it has two advantages over the latter: first, its low price; and second, the fact that it is so small that it can be carried in a pocket or handbag. This latter feature should make it of use to travelers who wish to carry a film collection of articles with them.

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GENEVA, N. Y.

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